languages of British Columbia. All this, it seems to me, argues in favor of the indigenous, American origin of the Eskimo.

A. F. CHAMBERLAIN.

University College, Toronto, Nov. 12.

IT seems to me that the similarities of sound mentioned in Mr. Chamberlain's letter cannot be admitted as evidence of a connection between the Eskimo and other American languages. The Eskimo words which he classes together are derivatives of entirely different stems, that cannot be traced to a common root. In the first table we recognize the following stems: *nipta-* ('clear weather'), *nipig-* ('to stick'), *nipag-* ('to vanish'). Under the heading *man* the words *inuk* and *angut* are classed together, although they have no connection whatever. In comparing languages, complicated derivatives must not be used, but the words must first be traced to their stems, and the meaning of the stems must be ascertained as well as the phonetic laws obtaining in the dialects of the stock, before it is possible to make a satisfactory comparison. Fortuitous coincidences of sound like those given by Chamberlain cannot be admitted as evidence of relationship. F. BOAS.

New York, Nov. 25.

Rate of Change in American Languages.

THE letter of Dr. Beauchamp (*Science*, Nov. 18) opens an interesting linguistic question. My own impression is that the rapidity of changes in unwritten, at least American, languages has been overestimated.

Sagard, in the preface to his 'Dictionnaire de la Langue Huronne' (Paris, 1632), asserted that the Huron was constantly changing, so that in a generation or two it would seem like a new language. Two hundred years afterwards, Duponceau took Sagard's very imperfect book, tried it on some intelligent Hurons, and found that "the language had not undergone any essential change" (*Mémoire sur les Langues de l'Amérique du Nord*, pp. 444, 445).

In 1578 Jean de Lery printed his 'Histoire d'un Voyage faict en la terre du Bresil,' containing long conversations in Tupi. Three hundred years later, Dr. Nogueira republished these conversations, with their equivalents in modern Tupi. The differences are surprisingly small, — with proper allowances for dialect and varying phonetics, scarcely more than between Lery's French and the French of to-day (see NOGUEIRA, *Apontamentos sobre o Abañeenga ou Lingua Geral dos Brasis, Rio de Janeiro*, 1876).

I have recently completed a comparison between the Alagüilac of Guatemala, which is the most southern dialect known of the Nahuatl, by means of a vocabulary obtained in 1878, with that tongue as spoken in the valley of Mexico in 1550, preserved in the 'Vocabulario' of Molina. The separation of the two peoples could not have been less than four hundred years; but the divergencies are so slight that I could easily have believed the Alagüilac words to have been obtained by a German (my informant was of that nationality) in ancient Tezcuco.

Dr. Beauchamp, in referring to conflicting orthographies of the same word, points out a real but not the only cause of apparent without actual change in these tongues. He also touches on the confusion liable to occur from the natives forming diverse figurative compounds to express objects and ideas new to them. I was struck with this lately in comparing the expressions in the Lenâpé for 'faith,' 'regeneration,' 'repentance,' and such theological terms, as introduced, on the one hand, by the Moravian missionaries, and, on the other, independently, by the Anglican Church. They are usually totally dissimilar.

But a much more curious and important law underlies the apparent variability of many American tongues. I refer to that of 'alternating consonants' and 'permutable vowels.' In a number of these languages it is entirely optional with the speaker to articulate any one of three or four consonantal sounds for the same phonetic element. For example: he may at will pronounce the syllable *ton* either thus, or *lon, nol, rot*, etc., alternating the elements *l, n, r, t*, at will. I have little doubt but that something of the same kind obtained in ancient Accadian, which will explain why the same cuneiform character stands indiscriminately for the sounds *ku, tus, pun*, and *dur*; and the recent researches of Dr. Carl Abel on the phonetic modifications of the ancient Coptic radicals hint strongly at the prevalence of this peculiarity in that venerable speech.

In America, I name as special examples of this the Klamath and the Chapanec. But that these phonetic variations are within fixed limits, and do not involve the integrity of the language, is curiously proved by the last mentioned. Remesal, the early ecclesiastical historian of Chiapas, states that the Chapanec was introduced into that department from Nicaragua many generations before the Conquest; probably it was not later than the year 1300. Now, in 1872, my late friend, Dr. C. H. Berendt, collected in Nicaragua, from a few old Indians, the only survivors of their tribe who spoke its tongue, a number of words and phrases of a dialect called the 'Mangue.' A comparison proves it to have been beyond question a very close relative of the Chapanec, essentially the same in fact, though separated from it for more than five hundred years (see an article on the Mangue by me in the *Proceedings of the American Philosophical Society*, 1885).

As in the Turanian tongues, the Turkish, for example, there is a 'vocalic echo,' the leading vowel of the word forcing the others to assimilate to it in sound, so in some American tongues there is a 'consonantal echo,' the presence of one consonantal sound requiring more or less changes in the others. The Tupi, the Chapanec, and the Klamath offer examples of the 'consonantal echo,' while a certain degree of the 'vocalic echo' is observable in the Kiche and Cakchiquel.

These phonetic laws must be thoroughly understood and allowed for, before any one pronounces positively on the rate of change in American languages. DR. D. G. BRINTON.

Media, Penn., Nov. 23.

Amnesia.

THE cases cited in *Science* (Nov. 11, 18, pp. 232, 250) remind me of the following. Some twenty-seven years ago, a neighbor of mine (a young man of twenty-five or under), springing from the vaulting-horse in a gymnasium to catch the trapeze, fell, striking apparently upon his shoulders, and was taken up insensible. Consciousness soon returned, perhaps in a fraction of an hour, but there was no recollection of the few hours just previous to the fall. As recovery progressed, however, it was said that his recollections came down closer and closer to the time of the accident; and that in a week or less he could even remember taking the leap, though not his striking the mattress.

Whether it be common that the progress of recovery should thus lessen the period covered by the amnesia, might no doubt be learned from such data as many professional athletes could furnish. An athlete once told me how, some years before, he had fallen on his forehead in the circus, and had been taken up for dead. His recovery, I think, had taken several months. He could remember, not indeed the blow, but the sense of powerlessness with which, in mid-air, he had realized that "his balance was lost." But perhaps he did not say whether, a few hours or weeks after the accident, his recollections had come down so far. J. E. OLIVER.

Cornell University, Nov. 18.

THE cases of amnesia mentioned in *Science* of Nov. 18 recall in my own experience cases which may be of sufficient interest to be recorded.

When about fifteen years old, I went into a stable to stanchion cows for milking. About an hour afterwards I was found wandering about the yard unconscious, and bleeding profusely from wounds in the face. I have not been able to this day to tell how I was hurt. I have no recollection, beyond going into the stable and fastening a few cows. My hat was found under the cattle's feet. My front teeth were loosened, a hole cut through my lip, and my shoulder in front badly bruised. I was feeling well at the time, and have never fainted, and cannot refer the injury to that cause. The nature of the injury would indicate that it came from the front, and must have appealed to my senses in their normal state.

From other experiences I have always believed that it is more common to remember the cause of an injury producing temporary unconsciousness than to forget it. I became unconscious once from drowning, but remembered vividly every thing when restored. I was once prostrated by lightning, but remember having seen the flash.

I think one's remembering the cause of an injury depends largely

on whether it appeals strongly and for some time to the senses, especially of sight.

I know, from personal experience and observation, that it is not uncommon for man to temporarily lose the power of the senses under excitement, while the body still performs its normal functions intelligently. Perhaps this would explain the third case mentioned by Mr. Hall. The gentleman remembered the runaway, but became so excited in checking the horse that his senses were oblivious to all surroundings.

In my own case the cause evidently acted before my eyes, and I have been led to believe that the cause of an injury may act so suddenly as to produce unconsciousness before the impression made on the senses can reach the brain.

Another case was unconsciousness produced by poisoning with sulphuretted hydrogen. I went into the attic to regulate a generator, and shut the trap-door, as I had to pass over it to reach the generator. There was but one window in the room. It was down, and about fifteen feet away. There being no gas at the hoods in the laboratory led me to think the iron sulphide was out. I disconnected the tubing, and found high pressure, which forced several gallons of gas into my face. It produced involuntary respiration, and my lungs were drawn full. Deeply impressed that my only hope of life was fresh air, I started for the window at once. Almost instantly I began to get dizzy, and my vision was strongly impaired. The window, only a few feet away, seemed very remote, and no larger than my hand. My rapid advance toward it gave me the strange impression that my legs were half a mile long. I became unconscious before reaching the window, and all is a blank until I found myself rushing down the stairs, two stories below, still impressed with the necessity of reaching pure air. In an unconscious state, I raised the window about eight inches, raised the trap-door, and fell headlong down the stairs to the laboratory, and was found by one of the students deathly pale, the blood settled under my eyes, my muscles rigid, and large drops of cold perspiration on my face. Soon after the student reached me, I began to show signs of recovery, and suddenly sprang to my feet, exclaiming, "I must have air!" and rushed down the stairs, regaining consciousness on the way.

This case shows suspension of a train of thought which was taken up where it was left off, and pursued after a season of unconsciousness. It also shows several intelligent trains of thought pursued in the absence of general consciousness, leaving no impression on the memory. I have often asked myself what directed me to raise the window and the trap-door, and have wondered whether there are centres in the brain to direct intelligent action for self-preservation in the absence of consciousness.

My intention was to open the window for air, but I have no knowledge of having done it. This has led me to ask whether impressions made on the brain during consciousness may not be automatically executed after the avenues to the external world are closed. May not a state of partial unconsciousness, as in somnambulistic sleep, be produced by injury, and well-directed trains of thought be executed and leave no impression on the memory?

A friend of mine has a blank of three weeks in her life while sick with typhoid-fever, yet was unconscious only the last ten days of the time. I have always explained such cases on the basis that bodily condition has much to do with the indelibleness of impressions made on the brain. When the body is weak, the impressions are weak and forgotten. Even in a state of health there are many perceptions that make no lasting impression. F. L. HARVEY.

Maine State College, Orono, Me., Nov. 22.

American Microscopes. — A Complaint.

EVERY autumn when the colleges and medical schools of the country begin their academic years, there are many students who come to their instructors seeking advice in regard to the purchase of microscopes. Often they appear already furnished with an instrument of which they are anxious to learn that the lenses are particularly good.

As it has been my duty for several years to conduct a large class in practical histology, I have had frequent applications for advice about microscopes, and have seen and examined a great many different stands, and the lenses of many manufacturers. I have had therefore, opportunities to test the practical convenience and advantages of the many sorts of microscopes which the students have brought along with them. The result of this experience is the conviction that it is undesirable to recommend a student to purchase any microscope whatsoever of American manufacture, and to always counsel him to obtain, if possible, one of the German or French instruments.

In order to make my judgment more clear, I may add that I know of no American microscope which I should like to purchase at any price, for my own use in histological or embryological work.

I venture to express this adverse opinion in regard to American microscopes in the hope of inducing some of our opticians to manufacture a stand for a microscope suitable for the use of students of histology and biology. It appears to me that the simple model now almost universally adopted in Europe is far superior to every thing offered us in rivalry to them by our own dealers.

To justify myself, I should like to give, first, the reasons for my disapproval of the American forms; and, second, the reasons for my preference of German forms. The fundamental error in microscopes of American manufacture is that they are for the most part constructed with a view of, I might almost say, entrapping inexperienced purchasers. The zeal of the maker is turned too much to decorative lacquering and nickel-plating: he adds to his stands as great a variety of mechanical contrivances and adjustments as the price of the stand will permit, and many of these contrivances are not really commendable for their utility. In the majority of cases the stands are made to tilt, which, for one that uses the microscope for real work, is an almost useless luxury, because he who really works in histology necessarily examines fresh specimens in fluids, or at least constantly has on the stage of his microscope preparations in various stages of unreadiness, and not mounted in a permanent form. All this implies the constant use of fluids, and, if the stage of the microscope is inclined, the use of liquids is impracticable. Any one, therefore, who uses his microscope for the ordinary purposes of a student or an investigator, or in connection with clinical or pathological work, very soon falls out of the habit of tilting his microscope. Hence it is, that, while making a microscope to tilt renders it considerably more expensive, it adds nothing essential to the convenience of the stand for laboratory work. This same fact, that most of the work must be done with the tube of the microscope vertical, renders it indispensable that the microscope should not be too high; so that we must put down the ten-inch tube as a bad feature for a student's microscope. A rack and pinion is undoubtedly advantageous: it renders the use of the microscope more convenient, and increases its durability by diminishing the strain upon the stand during the coarse adjustment of the focus. When this adjustment is effected by shoving the tube with the hand, the microscope wears out sooner than with the rack-and-pinion movement; yet even the rack and pinion, which are so generally put on our American microscopes, are not indispensable, and the greater part of the histological and embryological investigations of the past twenty years have been made without the employment of this convenience.

The stage of the American microscope is very faulty. The large movable glass plate with a hole through it is a toy fit only for an amateur or fancy collector: it interferes with the use of fluids, and with the freedom of movement of the slide over the field of the microscope, — the two things which are most indispensable in practice. A good stage should be large and flat, with nothing upon it except a pair of spring clips and a hole for a diaphragm. The diaphragms are often a matter of particularly fanciful construction. Thus the Iris diaphragm is often introduced to allure the inexperienced, but it is not a good form except in conjunction with an acromatic condenser. There are other details of construction which are equally open to unfavorable criticism, but it is unnecessary to go into their discussi on.

Unfortunately, while we see so much pains expended upon the brass-work of the microscope, we see a neglect of the optical members of the instrument; so that the microscope as a whole is converted into a showy piece of apparatus, and the eye-pieces and objectives are generally, though not always, of a decidedly inferior character: when they are really good, the lenses are very expensive.

If, now, our manufacturers would reverse the distribution of their